TRANSCRIPT OF PROCEEDINGS

ENVIRONMENTAL QUALITY

PROGRAM REVIEW

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ENVIRONMENTAL QUALITY

PROGRAM REVIEW

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615 S. FREDERICK AVE., SUITE R-2
GAITHERSBURG, MARYLAND 20760
PHONE: (301) 840-1996

APPEARANCES

Mr. Wendell G. Ayers
EQPO

Dr. H. G. Reichle, Jr.
LaRC

Dr. J. S. Levine
LaRC

Dr. R. E. Hughes
Bionetics Corporation

Dr. R. W. Stewart
GSFC

Dr. L. K. Peters
University of Kentucky

Dr. W. L. Chameides
University of Florida

Dr. G. L. Gregory
LaRC

Mr. D. S. McDougal
LaRC

Mr. M. S. Shumate
JPL

Dr. R. T. Menzies
JPL

Dr. T. T. Watson
JPL

Dr. E. V. Browell
LaRC

Dr. F. S. Mills
ODU

Mr. C. L. Korb
GSFC

APPEARANCES cont.

Dr. M. L. Spaulding
University of Rhode Island

Dr. C. H. Whitlock
LaRC

Dr. R. T. Gedney
LaRC

Mr. R. J. Blackwell
JPL

Dr. J. W. Campbell
LaRC

Dr. F. H. Farmer
LaRC

Dr. R. W. Johnson
LaRC

Dr. J. L. Mueller
GSFC

Mr. W. F. Croswell
LaRC

Mr. U. R. Barnett
KSC

Mr. H. J. Curfman
EQPO

Dr. I. S. Rasool
NASA

P R O C E E D I N G S

SESSION IV: TROPOSPHERE

Global Tropospheric Research

(Presentation of paper on the Measurement of CO and CH_4 Vertical Profiles by Dr. H. G. Reichle, Jr. (LaRC).)

QUESTIONS AND ANSWERS

DR. TILFORD: Does anybody have any data that you can compare it with?

DR. REICHLE: Siler. Not that we know -- Leroy Hite works very closely with him and we discussed this with Leroy and he didn't know of any data that he had, and Siler was going to talk to him when he went back to Germany in October and exchange samples.

We are trying to calibrate all of this thing, and as far as we know we don't. The only record that I know of as of a month or so ago, was that Hite had more or less continuous measurements except for a period of about a year when he didn't measure it because his bosses told him that since methane didn't change in the atmosphere there was obviously no reason to measure it and he didn't.

And, when he started again it had increased, and since then periodically he measures it and has been continuously increasing it, and he does have a continuous record of his calibration to gas, which NRL does not. So,

dle-2

we think it is real. Yes?

DR. STOLARSKI: When Peter Aholt came through here and gave a seminar in which he discussed the possibility that methane increases the same way, he was kind of circumspect about it, but he at least raised the possibility that he was getting 1.6 ish now and he used to get 1.4 when he was at Inghar (?) with Leroy.

DR. REICHLE: There was a lot of -- gee, there is a guy I sent a letter to yesterday. Hello Doug.

There was a real question as to were the measurements wrong, the old measurements, the new measurements or what, and we think they are right. Yes?

DR. DAVIS: Is it your understanding that Leroy Hites laboratory is the only laboratory right now that has a standard that can be traced back as far as about 1973?

DR. REICHLE: That is my understanding. That is my understanding of the situation now. NRL, Bob LaMountain published a lot of stuff back then, he had a 141 average, has not mainted the continuity of their calibration.

They lost their old calibration gas before they got the new ones, so they do not have continuity of their calibration.

As far as I know Leroy has the only continuous data.

DR. DAVIS: That is the one thing I think makes everybody just a little bit nervous.

DR. REICHLE: Yes.

DR. DAVIS: We really only have one calibration standard that we can trace back in time.

DR. REICHLE: Neither Siler or we were measuring at that time, five years ago.

MR. AYERS: Thank you Hank. The next paper, Joe you will be giving. Joe Levine from Langley, and it will be on the nitrous oxide measurement program.

(Presentation of paper on The LaRC Tropospheric Nitrous Oxide Program: A Progress Report by Dr. J.S. Levine (LaRC).)

QUESTIONS AND ANSWERS

DR. LEVINE: Any questions?

DR. TILFORD: Your seven times 10 to the 12th production rate, you say it is not linear. Is that based on the intermediate or high energy dissipation?

DR. LEVINE: It is based on the fact that over the limited energy range that we have, on this figure for example, that has to go from five to 12 of a factor of two or two and a half the production rate.

And, I call it production rate. There is 330 parts per billion nitrous oxide in the air, so I subtract that from 330, from each of these numbers since that was in

dle-4

atmosphere before we discharged.

And, it turns out that if you increase energy by a factor of two and a half you increase nitrous oxide production by more than a factor of 10.

DR. TILFORD: So, which number did you use to calculate your production?

DR. LEVINE: Oh, okay. At our maximum energy 17,000 joules, though, I didn't understand the question.

I calculated our numbers at 17,000 joules which is the highest energy we can work at the present time and, you know, we have to extrapalate from one or two times 10 to the four up to 10 to the eighth. So, I think that is the lower limit. Yes?

DR. DAVIS: Considering the complexity demonstrated in all forming nitrous oxide, green nitrous oxide lack of discharge, would you rule out the possibility that as you continue to increase energy you may be coming on linear but in the opposite direction?

DR. LEVINE: The only evidence I have is to the contrary over the limited range from two to 17, Over that small range we are increasing nitrous oxide production at a greater rate then the incremented--

DR. DAVIS: I would take your course of action.

DR. LEVINE: Yes.

DR. DAVIS: The other point that I would like to

dle_e

[illegible] is that after the calibrations of nitrous oxide, and I would like to know what your position is right now on this.

I know that there is some significant disagreement in the community, particularly against Ray Weiss (?) and some other individuals are disagreeing on the calibration of the 330 parts per billion.

DR. LEVINE: I am glad you asked that question even though I didn't ask you to.

This is the National Bureau of Standards experiment conducted last February. There were 15 laboratories the Cicero's, the Rasmussens, the McElroy's, you know, the people who are making a living doing this.

This is the -- okay two samples. We all got the same samples. The National Bureau Standards FXA-9 and FXB-9, all 15 samples were passed from the same gas.

The 15 laboratories in sample A ranged from 273 to 484 parts per billion. Sample B it ranged from 229 to 442, and this is a problem the fact that there is no primary calibration standard for nitrous oxide that the NBS has.

But, these are the guys who were in the field who are making a living doing this.

The mean for all of the measurements were for sample A 322, for sample B 290, and our determination, a

dle-6

Langley determination exactly 305 and BAQ 76.

So, I think compared to the range of these other people we look pretty good. But, there is a problem on the absolute calibration as shown by that bottom line, that the 15 laboratories had a very large spread.

What we are trying to do is, using the NBS standards and we calibrate with respect to the NBS models and if nothing else, we may not be right on the absolute value, we may be off 10 percent, but the relative variation, lightening is in fact producing an increase in nitrous oxide, and the absolute value may be off, but we ar producing nitrous oxide in the laboratory. Yes?

DR. DAVIS: That value there would correspond much closer to values being measured by Ray Weiss? Is that not true of 305?

DR. LEVINE: Yes, that is probably true.

DR. DAVIS: Because what Ray and now I think Yuter Heiss (?) contends, is that conceivably CO_2 is causing the interference in some of the analysis of using electron capture detection at a lower.

DR. LEVINE: Well, we can separate. In our system, at least in the Langley system, this peak is due to air plus CO_2. We can actually separate the CO_2 out of the air if we wanted to, but it takes more time.

And, this is clearly the N_2O. We don't have any

dlc-7

interference from CO_2.

What I think Scripts, I think their technique is to make the ratio of N_2O to CO_2. That is all of their data, and so they have got a ratio of N_2O to CO_2, and there is some question as to what the level of CO_2 is.

We don't do that. We measure N_2O by itself. Bill?

DR. CHAMEIDES: This is a comment on this energy orders of magnitude. I don't think it is that large. I think the important parameters as far as energy is concerned for lightening is the joules per meter that you put into the spark. You are talking about a linear spark.

And, I think you are closer. The lightening value is supposed to be around 10^4, 10^5 joules per meter, and you are really not that far from there. It is not orders of magnitude.

DR. LEVINE: Yes, in the gradient that is true. Yes. In the energy per length our facility is very close to real life magnitude.

DR. CHAMEIDES: I think that is the preliminary for everything. I think.

MR. AYERS: That would indicate then that by extrapalating linearly that you may not be too far off on the real value.

DR. LEVINE: Yes, if that is true.

dle-8

MR. AYERS: We will shift gears a little right now and go into some of our modeling activity and the first of a three paper series here will be from Dr. Dick Stewart from Goddard.

(Presentation of a paper on the Latitudinal Variations of Tropospheric Gases by Dr. R.W. Stewart (GSFC).)

QUESTIONS AND ANSWERS

DR. DAVIS: That is the overall computation, 1.5 parts per million?

DR. STEWART: No.

DR. DAVIS: 1500 parts per billion.

DR. STEWART: Yes, that is right. Okay, the units are wrong.

DR. DAVIS: Yes. It is .15.

DR. STEWART: .15.

DR. DAVIS: What was the level of NO that you used in generating your photochemical ozone?

DR. STEWART: The level of NO --

DR. DAVIS: It carries obviously --

DR. STEWART: Yes, it was typically throughout most of the troposphere about .02 parts per billion. 20 or 30 parts per trillion.

DR. STEWART: 20 to 30 parts per trillion?

DR. STEWART: Yes.

DR. DAVIS: Were you aware of the -- well, the

dle-9

game tag results during 1978, at least on the aircraft, indicated that the NO was less then about 30 parts per trillion and the instrument was not capable of measuring lower then that, our aircraft conditions.

However, a NOAA group out in the South Pacific, they were out there I guess about a month or so, their measurements indicated the NO was running more about three to four parts per trillion.

What sort of impact would that have on your calculating the low temperature ozone?

DR. STEWART: Well, I think the major impact isn't on ozone. We really can't say. The ozone budget looks pretty good to me.

I think the major impact might be on the carbon budget and on the proposed mechanism for supporting the oxidation of isoprene and turpenes to CO.

But, of course NOX is NO plus NO_2. Typically in the model though you have maybe two to five times as much NO_2 as NO. In the model results it is really the sum of the two that is important.

So, if you say there is three to four parts per trillion NO, you probably couldn't have more than 10 to 15 parts per trillion NOX and that is pretty low. It would cause difficulties in all of these budgets.

DR. MENZIES: You mentioned earlier that chemical

dlc-1

luminescent ozonesonde measurements might be a little bit low. On what experience do you base that conclusion?

DR. STEWART: On what experience do I base it?

DR. MENZIES: Yes.

DR. STEWART: None of my own. That is simply an argument that has been given by Chatfield and Harrison in some papers in "Geophysical Research","The Journal of Geophysical Research."

They have argued that in the troposphere the chemical luminescent sondes may give values that are about up to 50 percent to low when compared with the electric ozonesonde results.

And, Ernie Hilsenrath has recently looked at his data and he has seen that same kind of difference between the two types of measurements, so it is probably a real thing.

DR. MENZIES: Does that depend on altitude?

DR. STEWART: Pardon?

DR. MENZIES: Does that depend on altitude?

DR. STEWART: Yes. The problem doesn't exist in the stratosphere to the same degree.

MR. AYERS: Okay, I think we will take one more question here. Could you identify yourself?

MR. DUBIN: Murray Dubin. The nitric acid content in rainfall is a fairly good measure of the production loss

dle-11

mechanisms because of efficiency in rainout. But, have you found that the rainfall precipitation data, which has been going on for many, many years, shows the latitude dependence that you showed in your curve?

I have been looking at it and I don't find any industrial perturbation in the northern hemisphere.

DR. STEWART: In the --

MR. DUBIN: Except around cities.

DR. STEWART: The nitrate concentration?

MR. DUBIN: Yes.

DR. STEWART: Well, as I say there is the problem that the nitric acid lifetime is four to seven days. So, the question is, in a zonally average model what is it that you are representing?

I think that in the northern hemisphere it is really not a true zonal average. But, if you are using the full industrial source function it is nitric acid which is more typical of urban influenced continental air.

So, if you have remote rainfall results in the northern hemisphere I wouldn't be surprised if they don't show the nitric acid distributions.

MR. DUBIN: Well, let me ask it another way. If you take southern hemisphere mid temperate zone rainfall, do you find the large anomaly as you showed in your northern hemisphere, sub hemisphere difference?

dle-12

Do you show this huge industrial input of NOX, and that should show up in the nitrate measure, the nitrate in rainfall.

DR. STEWART: It should in the northern hemisphere is what you are saying?

MR. DUBIN: Excuse?

DR. STEWART: It should in the northern hemisphere is what you are saying?

MR. DUBIN: Yes, as you have shown in your slide.

DR. STEWART: What is the question, have I looked at that?

MR. DUBIN: I have looked at it and I haven't seen that big a difference. But, if the data is erratic I wondered if you had seen it?

DR. STEWART: Okay. I will accept that comment. But, what I am saying is that you have to interpret the models results and you have to look at where these rainfall samples were taken.

I haven't done that, so I will accept your comment. The question I am addressing is how do you interpret the model results.

MR. AYERS: Okay. Thank you Dick. Let's move on. The next paper here is by Dr. Peters from the University of Kentucky and he will be addressing the utilization of remotely sensed data for validating a global model.

dle-13

(Presentation of paper on Software Development for Model Validation Studies Utilizing Remotely Sensed Data by Dr. L. K. Peters (University of Kentucky).)

QUESTIONS AND ANSWERS

DR. PETERS: Are there any questions?

(There were no questions asked.)

MR. AYERS: Okay, thank you. The last paper in the modeling series and before the break will be by Bill Chameides from the University of Florida.

I would remind you that we did want hard copies of the viewgraphs. And, Lorraine have you collected any? A few. If you would just give them to Lorraine there we would appreciate it.

(Presentation of paper on Photochemical Impact of Anthropogenic Emissions on CO and NO_x by Dr. W.L. Chameides (University of Florida).)

QUESTIONS AND ANSWERS

DR. TILFORD: You say all of those are one percent?

DR.CHAMEIDES: One or two percent. Rich might have more to say about that later, but that is what I think we got.

MR. DUBIN: But, you have water in that mixture.

DR. CHAMEIDES: I am sorry, say that again.

MR. DUBIN: You have water in that mixture.

DR. CHAMEIDES: Yes, yes. Interestingly enough --

dle-14

MR. DUBIN: You said CO_2.

DR. CHAMEIDES: Well, we have CO_2 and water. We start with an atm sphere of CO_2, water, N_2, O_2, and methane and everything else is at such low concentrations that it doesn't matter because it is adequate serving process.

Okay? And, then you just calculate these. We have something like 30 or 40 species in here calculating the equilibrium concentration at every temperature.

MR. AYERS: Question?

DR. LEVINE: Bill, let me mention that Jim Hoyle at Langley is going to begin daily measurements of the vertical distribution of ammonia using his infra-red hetodyne (?) radiometer, that is the same instrument here. Within a month he will be making --

DR. CHAMEIDES: Well, that curve was addressing that experiment.

DR. LEVINE: Yes, that is right. But, he will do it on a routine basis.

DR. CHAMEIDES: That is a very good development.

MR. DUBIN: A question on the water vapor content. How would you incorporate the high water vapor saturation condition of water droplets that occur in storms, the certain hydroxyl --

DR. CHAMEIDES: We feel that the heterogeneous, possible heterogeneous process would go on. At these high

dle-16

temperatures you are not going to have a -- At equilibrium certainly at these temperatures you are not going to have a water drop. The possibility of --

MR. DUBIN: You say you have a saturation water condition throughout the region of the discharge?

DR. CHAMEIDES: No, no. What we start with is an amount of water vapor, ambient concentration of water in the atmosphere.

And, we can increase that or decrease that depending on, you know, what concentration of water vapor you want.

What we can't include, obviously, is the possibility that you have some heterogeneous processes occuring on water droplets on condensation nuclei, that might be enhancing the production of NO or N_2O.

I think that possibly heterogeneous processes might be quite important for N_2O, and a global source of N_2O might be much higher then in fact these calculations indicate because we can't view those heterogeneous processes.

Interestingly enough the equilibrium concentration of NO is completely independent given the range of water vapor that you might get in the atmosphere.

The equilibrium NO concentrations are completely independent of how much water vapor you have. That is true of N_2O also.

dlc-17

MR. AYERS: Okay, thank you Bill. It is break time. We are pretty much on schedule. I think if we come back at 10:30 A.M., which will shorten the break a little bit, we won't have to cut into the lunch hour.

(Brief recess.)

Regional Tropospheric Research

The 1978 Southeastern Virginia Urban Plume Studey (SEVUPS)

(Presentation of a Program Overview by Dr. G.L. Gregory (LaRC).)

(Presentation of the 1978 SEVUPS Data Analysis and Plans by Mr. D.S. McDougal(LaRC).)

QUESTIONS AND ANSWERS

MR. AYERS: Pardon me, Dave, what are the times?

MR. MCDOUGAL: These are at time of day, at noontime into the afternoon, starting at eight in the morning, eight, 10, 12, two.

(Presentation of the JPL LAS Operations, 1978-79 SEVUPS by Dr. R.T. Menzies (JPL).)

QUESTIONS AND ANSWERS

DR. MELFI: Over the altitude that you make?

DR. MENZIES: Yes, these circles are mean values, right, assuming we are mixed. The spatial resolution here is about 10 kilometers. These are points of averages over two to three minutes of flight time. Okay?

And, again these processes just indicate the

dle-18

in situ and the ambient ozone gradients.

DR. MELFI: What altitude were you flying?

DR. MENZIES: We were flying at 3500 feet, which most of the day was slightly above the inversional air. However, at this point I think we were within -- we probably were a little bit lower then the base of the oversion.

(Presentation of the 1979 Field Experiments by Dr. G.L. Gregory.)

(Presentation of paper on the Oxidation Mechanisms of SO_2 in the Urban Plume by Dr. R.T. Watson (JPL).)

QUESTIONS AND ANSWERS

MR. AYERS: Are there any questions? Doug?

DR. DAVIS: Did you say you were or were not going to study HO_2 as function pressure? That last slide indicated you were not going to do function pressure, but your first slide of your talk said you were going to get into function pressure.

DR. WATSON: Funny you noticed that.

(Laughter.)

DR. WATSON: If the flash photolysis system works for HO_2, which it should in theory work, then we will do it as a function of temperature and pressure.

We can certainly do it in a low pressure regime by the molecular beam mass spec system. If the flash photolysis system works for HO_2 we will also do it which

le-19

then gives us a function of pressure and temperature.

The first system will only be the low pressure system, but we will follow it through.

DR. DAVIS: Do you have any idea whether or not Carlton Howard has looked at that reaction?

DR. WATSON: No, he definitely hasn't.

MR. MUGLER: What is the schedule for your equipment completion?

DR. WATSON: The equipment is completely, it is complete. It is ready to do that study. There is a combination doing sulphur chemistry and chlorine-bromine chemistry or the one that has shoved these others out, from the ones he has given me.

So, the sulphur chemistry to start with can be -- that is what I got what goes there.

(Laughter.)

DR. WATSON: So it is a question of if we are going to do methyl peroxy plus NO within the next three weeks, methyl peroxy plus SO_2 in the next month, and then we will carry it on from there.

MR. AYERS: Okay. Thank you, Bob, for a wide range. The next speaker told me that he has measured SO_2 and aeorsols in a power plant plume, but in light of your paper I am waiting with baited breath to see what he will talk about.

dle-20

(Presentation of paper on the Measurements of SO_2 and Aerosols in a Power Plant Plume Using a UV/DIAL by Dr. Browell (LaRC).)

QUESTIONS AND ANSWERS

MR. MUGLER: You say that is the centroid of the plume?

DR. BROWELL: Yes, this is the centroid.

MR. AYERS: There seems to be a plume coming from the right stack. Is there one coming from the left also?

DR. BROWELL: The plumes are very clean and they are very difficult to detect visually and even in photographs.

DR. MILLS: There was only one plume at the power plant at the time we were up there.

MR. AYERS: One plume.

DR. BROWELL: One plume at the time we were up there. That is right.

DR. MELFI: And you are seeing one plume.

DR. BROWELL: Some of the data -- We were using a very simple model just assuming that we were looking at a cylinder of gas that was rising and having a particular diameter of 150 and assuming that out of most of those scans we were having a peak value of about 1300 parts per million meter.

So, you are right. I thought it was very, rather

dle-21

coincidental that we came that close.

MR. AYERS: Thank you Ed. Let's see, the last paper then for the morning and in the tropospheric session will Be Dr. Larry Korb from Goddard here, and he will bring us up to date on the gas filter instrument that he has been working on for a while.

Advanced Techniques Development

(Presentation of paper on the Development of a Differential Correlation Radiometer for Tropospheric Pollutant Measurements by Dr. C.L. Korb (GSFC).)

QUESTIONS AND ANSWERS

MR. AYERS: Larry, what wave length measurements are you talking?

DR. KORB: Okay most of the measurements come out between 2.2 and 2.3. The detectors we are using in the inteminites (?) that go out to 5.5, thinking that HCL we would be making measurements in three to five region.

If we want to look at SO_2 we will go out to beyond four.

So, for the field model we can go out -- for the field model we will be working at the instrument solar radiance.

With this here, with this strong signal we feel, you know, considerably longer wave lengths then we could if

dle-22

we are using an aircraft model, a space craft model where you are looking down, and I think expect it to a region short of about 3.7.

MR. AYERS: This is your field model?

DR. KORB: Yes, this is our field model.

MR. AYERS: Are there any questions? Dave?

MR. HINKLEY: Larry, yesterday Jim Russell described in a little detail some of the gas cells he has been developing for the HALOE program.

Have you, since you had some problem, have you talked to Jim about them, because they are about the same wave length.

DR. KORB: No, I have not. The problems we have had up to this point, we are into first stage problems. We have not seen the end of our problems yet.

But, the problem of the in the CNS come in fueling the cells. The next order of that would be in terms of cells leaking, and how well they can contain the gases. Yes, we will contact him.

DR. WATSON: Instead of a carousel of fixed pressure gases, could you use something like one of these pressure modulators used by Glasgow and such? Then you have got totally treatable pressure all of the time.

DR. KORB: No. Yes, you could. Let me comment on the differences between let's say this and that.

e-23

If you use the PM error you can't sweep the pressure. However, you are restricted to using fairly low pressures, say, on the order of perhaps 25 millibars.

In turn, in this instrument just studying the range restricted to the 45 to 50 millibar range, OH has 1000 more bars which in turn allows us to go into the line of files in the troposphere, and get out into the wings of the line profile in the troposphere.

So, with a pressure modulated system you could tune across the profile in the stratosphere. However, if you wanted to get down in the troposphere you would be able to get to much higher pressures.

You have a very large pressure differential between these two cells.

MR. AYERS: Hank Reichle?

DR. REICHLE: In your last statement I don't believe it is quite true for all gases in the troposphere, do you think you can use HCO in the lower troposphere by going to a multiple pass cell with the low pressurization involved for other gases.

What you are saying is true, we have not looked at that problem.

And, the question, did I understand that you are going out five and a half micrometers with this system?

DR. KORB: The detector will go out to five and

dle-24

a half.

In terms of measurement, with the exception of SO_2, we don't plan to go beyond about 3.7.

DR. REICHLE: Do you intend to use, at 3.7, do you intend to use that in direct solar mode or in a native viewing mode you can deflect the radiation?

DR. KORB: Certainly the former. In the latter case we will attempt to use it in a native viewing mode under two different types of conditions.

One, direct native viewing, looking at diffuse refraction; two, in a downward looking mode but using solar glitter off the ocean, which in turn will certainly put us into a solar reflected -- strongly into the solar reflected region and will greatly diminish the effects at the terminal.

DR. REICHLE: The rating functions that you show in the calculations, d I understand you correctly, are just using the transmittance part of the one dimensional equation of radio transfer, and did not involve the surface temperature and the gas temperature?

DR. KORB: That is correct.

MR. AYER: Okay, I have a question. On the schedule when do you think the field model will be ready to be in the field?

DR. KORB: Okay. We hope to have it starting field

lle-25

measurements in June.

MR. AYERS: Of this year?

DR. KORB: June.

MR. AYERS: I would encourage you to work with Jerry Gregory and the group at Langley on the urban plume study so you have the factor, factor N on the graph.

Okay, if there are no more questions this brings us to the end of the best part of the program.

(Laughter.)

MR. AYERS: And, I have one viewgraph here showing some of the things that you did not see this morning. What I have simply done is gone back through the ARTOPS (?) and the AN's, and this is some of the things that we did not have time for presentations this morning.

When I made up the program I had about three days of presentations and they beat me back to the time we had on the program.

But, Joe Levine who did give a paper this morning is also working in some photochemical model studies at Langley.

Casey Jachimouski, who was here this morning, has a group at Langley that is doing some laboratory chemistry with -- Casey is here I believe. I don't see him. There he is. I see a hand.

But, I think primarily the emphasis is on, what,

dle-26

heterogenous chemistry, soot, and SO_2 interactions. That is one of the focuses of the study.

Pat McCormic, who has an aerosol group at Langley, is being funded under this program to do fundamental studies leading to monitoring of aerosols.

Sherm[illegible] Beck, who works with Hank Reichle at Langley is working on gas correlation of the analytical techniques, the data analysis techniques, particularly aimed at the MAPS, OFT-2 MAPS instrument.

I am trying to avoid acronyms, but I hope most people understand what experiment that is. That is the global CO monitoring from an early shuttle flight experiment that Hank heads up.

Bill Chameides and one or two others are here on the science team. Lynn Peters is also on the science team there.

We have a flight instrument division or a flight experiments division I guess it is now at Langley. They are looking at paths to promote essentially correlation techniques with emphasis on two developments.

One is the pressure modulated radiometer technique that you just heard a question about from a previous speaker, and there is some looking at use of that technique for tropospheric measurements.

Also, the correlation interferometer technique.

dle-27

You may know that instrument as the COAT or the Simat instrument. We didn't cover either one of those this morning.

There was mention of the DACOM experiment or instrument. The differential absorption of the CO monitor. That is the instrument that they have used for CO measurements and also the nitrous oxide measurements that Joe Levine mentioned.

Ed Prior, at Langley, has done some very interesting work using the Wraps (?) data from St. Louis, the EPA data from St. Louis, and looking at empirical model development where they use the data on the precursors, fits it to a simple empirical model and they found some very close correlations , and it seems like a very powerful tool for predicting ozone concentrations based on previous data and is strictly an empirical approach as opposed to the theoretical photochemical approach.

Tom Wakelyn, who is here today, also has been involved in chemical modeling associated with the southeast Virginia plume study.

And, what he has -- Tom correct me if I am wrong, but has essentially particular rised some of the current bottling techniques that have been developed in the urban photochemical modeling community, but has particularized it to the Norfolk area and it will be used as a tool in

the southeast Virginia study.

Finally we have an AN sponsored piece of research at the -- at ERT, Environmental Research. Jim Barnes, what is the name of your company other than ERT?

MR. BARNES: Evironmental Research and Technology.

MR. AYERS: Environmental Research and Technology Incorporated. Jim Barnes, who just spoke, has been looking at current satellite images, particularly from meteorological satellites.

Agos, and the Tyros series, and the defense meteorological satellite system to looking at visible evidence of what we call, in the trade, hazy blod or aerosol in the atmosphere on a regional scale, and how they may correlate with high sulfate episodes in the northeast, and particularly the northeastern part of the United States.

There is currently in operation a network called a Sure network for measuring sulfates at ground level over this region, the northeast part of the country.

That work is sponsored by EPRI, which is the Ecologoical Power Research Institute. So, in this particular study at ERT we are looking at satellite imagery, looking at the data, the sulfate data from the Sure network, and basically seeing how -- what we can see in the satellite imageries that would help us in understanding the production

dle-29

and transport of sulfates in the atmosphere on a regional scale.

So, with that I would like to conclude the tropospheric part of the program. I would like to say that I am very gratified, personally gratified, from what I have heard this morning.

It seems to me that this particular part, the tropospheric portion, is tremendously exciting and also I am very pleased to see the interaction between the various researchers in the different parts of the program. Let's go to lunch.

(Whereupon at 12:30 P.M. the meeting recessed for lunch, to reconvene at 1:30 P.M. the same afternoon.)

dle-30

AFTERNOON SESSION

SESSION V: WATER

(Presentation of paper on Laboratory Spectra and Optical Physics Research by Dr. C.H. Whitlock (LaRC).)

QUESTIONS AND ANSWERS

MR. CURFMAN: Are there any questions of Charlie?

(No response was heard.)

MR. CURFMAN: All right, I would like to go on then and we have had a very successful activity in the program area in the Great Lakes that has been conducted by the Lewis Research Center. Dr. Richard Gedney will present some of the results of that activity. Dick?

(Presentation of paper on Water Quality Research on the Great Lakes by Dr. R.T. Gedney (LeRC).)

QUESTIONS AND ANSWERS

DR. MELFI: Dick was that a unique solution or was there a number of different combinations that could have done just as well?

DR. GEDNEY: No. There is only one. It is a non linear equation and there was only one in this case. My math modeling work didn't make it. Our work, math modeling anyway.

I just want to say that in the math modeling area, I just picked this up --

(Laughter.)

dle-31

SPEAKER: That wasn't my question.

DR. GEDNEY: Since 1969 we have been working with Case Western Reserve and EPA on the modeling work.

We have done quite a bit. We never reported it. In the old days it wasn't looked upon as the proper thing to do, but we always thought it was, and we have a lot of results from that area.

MR. CURFMAN: That is in your handout?

DR. GEDNEY: Yes, it is in the handout.

MR. CURFMAN: I didn't really think he was going to take all of Mal's time, even though he told me he would, when he found out he wasn't here. Bob, go ahead.

DR. JOHNSON: Johnson from Langley. Dick, the model results that you show seem to be very similar to some results Earman had six or eight years ago I think in the program they called the ALCAN (?).

Have you compared your radiance models with theirs or with some other models?

DR. GEDNEY: Which models, in the water or in the atmosphere?

DR. JOHNSON: I believe that these were surface level, upwell radiance poured suspended solids and phytoplankton, and I noticed you had two curves in the fuel water.

DR. GEDNEY: Well, these were in actual lake

dle-32

conditions. The people have measured upwelling radiance before, reflectants before.

I don't know of anybody who has successfully gotten the radio transfer model to predict those radiances up to this time.

DR. JOHNSON: This was calculated from a model using phytoplankton, using chlorphylls.

DR. CEDNEY: The models have been around a long time. Nobody has verified the models. I think the significance of this work is that it is the first time that I know of -- well, I mean it is not the first. I shouldn't say that.

But, it is only in the past year or two that these models are being verified in the field. I think that is significant.

The models, that model, the working model has probably been around for six years too. I don't know.

DR. SALSMAN: The air model -- Jack Salsman, NASA headquarters, Lewis at one time.

The air model that they used in the passing, used artificial optical parameters such as absorption of scattering They derived from measurements in the laboratory, but they were not the actual absorption of scattering inherent characteristics of the -- So that is a big difference.

DR. CAMPBELL: Jenny Campbell from Langley. The slide you showed about your radio transfer model, you talked

le-33

about A being absorption, B total scattering, are those coefficients or they go E to the A minus A something or --

DR. GEDNEY : Those are actual -- yes, you would put those in the exponential --

DR. CAMPBELL: Okay. So, they are absorption and scattering coefficients?

DR. GEDNEY: Yes.

MR. CURFMAN: Anything else?

DR. GEDNEY: I should say that this work is normally managed at Lewis by Jack Salsman.

MR. CURFMAN: One of the activities that has been going on for a couple of years and they are in the process of transferring some of this technology to the user, has been the activity that JPL has been conducting cooperatively with the EPA Las Vegas Lab in the lake classification activity. Dick Blackwell from JPL will discuss this.

(Presentation of paper on JPL/EPA Lake Classification Project by Mr. R.J. Blackwell (JPL).)

QUESTIONS AND ANSWERS

DR. SCHAFFER: Is that 180 feet or is it 180,000?

MR. BLACKWELL: 180. It is a very small base.

MR. CURFMAN: Jack?

DR. SALSMAN: How large a lake is Tahoe?

MR. BLACKWELL: About 12 by 20.

DR. GEDNEY: Gedney of Lewis. Do you know, is there

dle-34

a consensus on what the limiting nutrient is in this lake?

MR. BLACKWELL: It is dependent on the lake and the location. They found in some lakes it is phosphorus and in some lakes it nitrogen.

DR. GEDNEY: Yes.

MR. BLACKWELL: I would say the consensus is phosphorus.

DR. GEDNEY: Well, you know, some places it is nitrogen, some place it is phosphorous that explain some of your scattering and scatter plots.

MR. BLACKWELL: Well, we have also in one of the experiments we had with the nitrogen and phosphorous ratio.

DR. GEDNEY: Well, which one is limiting is important to how it will relate to the other problems, and with chlorophyll.

s-2

MR. CURFMAN: Our next paper will be presented in two parts, and Dr. Jenny Campbell and Frank Farmer, both from Langley, will discuss some recent experimental activities concerning the measurement of phytoplankton diversity and chlorophyll a using airborne fluorosensor. Jenny?

(Presentation of paper on the Field Experiment to Measure Phytoplanton Diversity and Chlorophyll a by a Remote Airborne Fluorosensor by Drs. J.W. Campbell and

dle-35

F.H. Farmer (LaRC).)

QUESTIONS AND ANSWERS

MR. CURFMAN: Questions, comments? Jack?

DR. SALSMAN: Since your system might provide us for some depth measurement, and you have depth in the equation, are you attacking that question at all and trying to get to how --

DR. CAMPBELL: Would you state that again?

DR. FARMER: May I answer the question?

MR. CURFMAN: Sure?

DR. SALSMAN: It looks like the possibility is there.

DR. FARMER: Yes, the possibility is there. It just means modifying the system really to do this in terms of the time study, and that is something we don't have the resources to do right now.

This is something that I will look at if we can do it but it may be a fiscal year 80 project.

MR. CURFMAN: I think you have to remember, Jack, that they are not looking at the laser return, they are looking 685 nanometer return only.

DR. CAMPBELL: The chlorophyll that we estimated a weighted mean, basically, for the column where you can write down the weighting functions which places a great deal of weight on the upper layers.

dle-3

It drops off rapidly as something like E to the Alpha Z, something like that.

DR. BROWN: Janet, please bring out the fact too that the system sensitivity, or the micrograms per liter capability is a function of the system in its present condition with a power that now exists.

DR. CAMPBELL: Did everyone hear that, that the system, everything we have said so far is based on the system in its present configuration. That is the sensitivity of it, and I am sure that a lot of that can be improved if we had greater power, you know.

We know a lot of ways we can improve the system, all you have to do is give us the money.

(Laughter.)

DR. MELFI: Janet, the data that Frank presented earlier which showed the profile as you pull up towards the problem, was that calibrated using the algorithm that you plot?

DR. CAMPBELL: Yes.

DR. MELFI: So, you needed ground for the total chlorophyll?

DR. CAMPBELL: We can use total chlorophyll from one station if we have to. I mean, we can actually use as little information as that, but we prefer to have obviously more because that reduces the error.

dle-37

DR. MELFI: So, in this particular case did you use total chlorophyll from each of the stations?

DR. CAMPBELL: Yes.

DR. FARMER: No, no. One station. All of that calibration was in one station.

DR. CAMPBELL: No, oh no. I beg your pardon.

MR. CURFMAN: Any other questions?

DR. CAMPBELL: Incidently, Howard, I am Malcolm Spauldings contract monitor, and therefore I feel that I can take his time.

(Laughter.)

MR. CURFMAN: Only if you are going to present his talk.

DR. CAMPBELL: Don't ask me what he was going to say, either.

MR. CURFMAN: Any other questions for Janet? All right, let's break now and we will get back again at 3.15 P.M.

(Brief recess.)

(Presentation of paper on Ocean Dumping Monitoring Research by Dr. R.W. Johnson (LaRC).)

QUESTIONS AND ANSWERS

MR. CURFMAN: Joe, go ahead Joe.

DR. DRURY: You mentioned that the shape of the curve, you said the shape of the curve was a quarter, and

dle-38

it looked like the Puerto Rican dump had a very similar shape to the acid this spatial thing at lower levels.

MR. MUGLER: The Puerto Rican would be darker.

DR. JOHNSON: Well, the current shape is similar.

DR. DRURY: It peaks in about the same points.

DR. JOHNSON: Yes. But, again this is the curves which indicate the ocean water.

DR. DRURY: Okay, so you mean the level of the curve is also important as well as the shape.

DR. JOHNSON: Yes, yes, right

DR. DRURY: Okay.

DR. JOHNSON: That is a good point.

DR. DRURY: Because you had mentioned shape.

DR. JOHNSON: Okay, good point.

DR. DRURY: The other thing, along the same line, was whether or not you had -- Charlie mentioned that they have different mixtures from these pharmaceutical companies and did you just -- you just did it at this one time.

Would you expect to get a -- well, you don't know. You haven't done the experiment except for that one dump, right?

DR. JOHNSON: A limited technology base.

DR. DRURY: Right.

DR. JOHNSON: At this point, Joe and --

DR. DRURY: Of course you could get an entirely

dle-39

different figure.

DR. JOHNSON: We have a maybe six to eight curves from the sewage sludge and the acid waste. We have multiple passes over one mixture of the pharmaceutical, over one mixture of the Galveston tube(?).

So, that the laboratory material supplement, or the laboratory information supplemented by the AF spectral is going to play an important part. Okay?

MR. BLACKWELL: Blackwell, JPL. Do you have any handle on the clinical parameters of the sludge in the acid and is the specific gravity heavier in the sludge then the acid? Is that the reason for the first one?

DR. JOHNSON: There are two components typically, and one of which is heavier then the water and sinks fairly rapidly.

I am glad you mentioned that because part of the oceanographic research work that we are doing includes acoustical studies by John Cronin (?) down at ALML, so that we have gotten some pretty good information on how it is distributed in the water column shortly after it is dumped.

Yes, there is a surface manifestation, but there is also a component of it that goes right down to the water column, and incidently some part of it just goes right down to the thermoplane (?) and spreads that out.

dle-40

So, that is part of our research. Yes?

DR. LEHMANN: You mentioned that from hours and days, what lasts for hours and days, the fact that you can see anything or --

DR. JOHNSON: The acid. Can you see anything more than several hours for the dumps --

DR. LEHMANN: What do you mean by hours and days, what is it you can see in the hours or days?

DR. JOHNSON: Oh, okay. The elapsed time after the dump in the case of the acid waste, we can see it for up to about three days at the dump site in the surface waters. Okay.

In the case of the sewage sludge the pharmaceutical waste and the material dumped in the galvesty (?), it seems to disappear in four to six hours.

So, if we came over the next day we wouldn't see any indication of the dump. Now, this is particularly important in the satellite based program because of the frequency of coverage.

In otherwords, if the satellite went over and they came in and two hours later and dumped it, and then the next overpass was several days, we would never know that a dump took place from the model.

MR. CURFMAN: Jules, maybe you remember seeing of the landsat images in place, and it looks like the remnants

dle-4

of previous dumps, but of course they are dumping several times a day up there, so you do see in some of those images remnants of previous dumps that have persisted for anywhere up to six or eight hours, apparently.

MR. MUGLER: Bob when you say see, do you mean see with a multi spectral scanner, no detectable signal, or do you mean see with your eye, or do they both happen to coincide?

DR. JOHNSON: I think that you have to say that the multi spectral scanner would much more sensitive, at least in the order of magnitude.

MR. MUGLER: And, when you say you can't see then you can't detect with the MSS?

DR. JOHNSON: The MSS. To detect with an electronic scanner is better. Yes, immediately after the dump you can visually see it, you know, from an ankria (?).

But, I think the landsat studies that Dick Clements, Dave has done with the additional data, indicates that is there and you can see it in the additional data when you couldn't --

DR. LEHMANN: You showed five or six sites. Are those all there? You showed five or six dumping sites.

DR. JOHNSON: Yes.

DR. LEHMANN: Is there any more then that, or is that it?

dle-42

DR. JOHNSON: No, that is all of them now.

MR. MUELLER: All of the active.

DR. JOHNSON: Yes. Okay, now remember that the material that is dumped in the largest quantities is dredged oil.

This is not part of the current program. In the future it will be because that is another thing for the ocean dump program.

But, at this point on the east coast and the Gulf of Mexico, those are the only active sites and they are controlled by law by EPA.

MR. CURRAN: Thank you, Bob. Our next speaker is going to be Dr. Jim Mueller from Goddard describing some of the work that -- I guess we have to say Jim that maybe it is the end of this work for the moment, the work that we have done relative to the detection of red tides. Jim?

(Presentation of paper on Red Tide Monitoring Research by Dr. J.L. Mueller (GSFC).)

QUESTIONS AND ANSWERS

DR. MUELLER: Any questions?

DR. SALSMAN: What about sky reflectants, skylight reflectants in the albedo? Are you going to lop off straight wave length independent term on that or --

DR. MUELLER: We assume that all of the surface reflectants for glitter, skylight reflectants are second order

dle-43

for this first cut.

I think if you go back and look carefully at that full scan measurement there is a pronounced -- if you look at what you think are relatively clean water areas out here,you can't see it in this picture, but if you look carefully at a good representation of this, if you look out here, versus out here in the Gulf Stream, there is a pronounced brightening over here that shifts.

The problem is associated partly with sun glitter, partly with the aerosol. It is really very difficult to pin down. And, this is good data from a cloud standpoint.

Most of the ocean seems to be cloud covered most of the time.

MR. MUGLER: Jim, could I conclude from what you said, particularly about this scene on the left, that CZCS is perhaps a better atmospheric turbidity measuring instrument under those conditions then it is an oceanographic instrument?

DR. MUELLER: No. I am saying that we can take this scene on the left, and you calibrate that scene, that is not calibrated, that is zero gained data. That is not calibrated.

If you calibrate that scene and apply the algorithm the water radiances come out very strongly.

MR. MUGLER: Okay, well let me twist it again then.

dle-44

You are taking the aerosol information, plus this noise and throwing it away?

DR. MUELLER: You could also back out an aerosol estimator. CZCS is a damned good aerosol detector, one of the best that you will ever see. We are seeing stuff that you just can't see at the island.

MR. MUGLER: Yes, Warren made the point on that plume on the left.

DR. MUELLER: Yes.

MR. MUGLER: That you didn't see it with those.

DR. MUELLER: Well you won't see this stuff with landsat, this fine thin hazy stuff, that is either plume cirrus, or some of us call it haze. I kind of think of it as organized cloud, but it is hard to say what it is, but it is definitely an aerosol.

MR. MUGLER: It is visibility degradation.

DR. MUELLER: Pardon?

MR. MUGLER: I say it is visibility degradation, no question where that is.

DR. MUELLER: But where it is in the atmosphere is not a question.

DR. HOVIS: When we process the data, where do we process the aerosol? The first level product that will be available is calibrated radiance from all the channels with no algorithm applied except calibrations.

dle-45

If you want to process for aerosols be our guest. But, we unfortunately can't afford to do both processes.

MR. MUGLER: Yes, I understand. But you do, in fact I guess you made the comment, you do suggest that there is certainly some meaningful aerosol information in there you can probably extract.

DR. HOVIS: You will see aerosol -- this has been which you will not see in any other, this is simply because the dynamic range is so different.

MR. CURFMAN: Remember the stuff Griegs (?) did. When he did it with landsat it was when he was looking at water.

MR. MUGLER: Oh yes.

MR. CURFMAN: And, he only played around with the first four or five gray levels that came off right at the very bottom of the calibration.

This basic approach to really see what is in the low reflecting stuff in the water is bound to accentuate those kinds of things that are atmospherically contributed.

DR. HOVIS: This instrument has about seven times the scan as landsat does. It saturates at one seventh of landsat roughly.

And, it also divides that one seventh into 256 levels instead of 64 as landsat does. So, it is much more sensitive to anything like this. That is why I say that the

dle-46

aerosol looking he might be better off using this then any of the others.

DR. MELFI: Jim, that picture that you just had up there, was that processed for land did you say? Did they process it to try to bring out the land features?

DR. MUELLER: That picture was a straight rock counts combination of one treatment. Nothing was done to the data except we went into the image 100, sub sampled it and put it all on the screen and spit it out for hard copy. Nothing done to it.

DR. MELFI: So, that means when you get images like that you are not only capable of getting your water image but it also resolves some land features.

I would have guessed that the sensitivity, Jim, of the instrument that the land would be washed out completely.

DR. HOVIS: It is in some bands.

DR. MELFI: But not completely.

DR. HOVIS: You experience complete saturation but land isn't very blue, and the blue band shows up.

DR. MELFI: Okay.

DR. MUELLER: Any vegetation pulls out the blue lights that are in there. In many cases you go around the Mississippi and the minimum brightness in channel one isn't other than that.

dle-4

DR. MELFI: So this is taken almost at the winter solstice

DR. SCHAFFER: This really what you call pretty much a multi disciplinary type of instrument.

DR. HOVIS: It wasn't intended that way, but it worked out that way.

DR. SCHAFFER: I just wanted to make sure some people heard that statement. That is all.

(Laughter.)

MR. CURFMAN: Thank you Jim. I would like to now introduce Bill Croswell from Langley to talk about the other half of the emphasis activities associated with ocean waste monitoring, and in particular some of the oil spill related activities. Bill?

(Presentation of paper on Oil Spill Monitoring Research by Mr. W.F. Croswell (LaRC).)

QUESTIONS AND ANSWERS

DR. GEDNEY: When you say space technology is it separate?

MR. CROSWELL: He wants to know whether space technology enters into the satellite. The mandate from Congress does not necessarily specify it from the satellite. Okay?

And, I consider that to be a very important point. Obviously we don't involve satellites , or the National

dle-48

Aeronautics and Space Administration, so we have got a problem.

Some of the things, I think Dick, honestly will be within the capability of the satellite and some will not. There will have to be some airborne sensor or some technology that we can recommend, some sensor. Okay?

DR. GEDNEY: So the contractors option is either aircraft or satellite?

MR. CROSWELL: Yes, overall system. Yes.

DR. KERSANDERS: Kersanders from EPA. Are you considering any kind of remote in situ sensors for the contractor to study as he goes through this process, or are you strictly restricting your field of study to airborne or satellite systems?

MR. CROSWELL: Remote, generally airborne satellite systems, what I call remote sensing, not in situ.

DR. KERSANDERS: Why?

DR. MELFI: I think he is talking about remotely interrogated in situ sensors. Is that right, John?

DR. KERSANDERS: The CB.

MR. CROSWELL: I understand that.

DR. KERSANDERS: The Coast Guard and DOT are doing studies in this area, and they are dropping devices around oil spills and coming back with a lot of good quantitative information.

dle-49

I was just wondering, should you be considering that or is this --

MR. CROSWELL: That is interesting. No, I was interested in -- I have really this study to what I call the image formation remote sensing part of it.

I have no argument that would be interesting. There are also all kinds of other more detailed chemical studies we could make and things of this nature.

No, I did not. Maybe that is one point I neglected. Maybe I could consider it a bit more. If there are questions about that study ask about that.

One of the things that came out of the meeting and this set of people was it was very important and a very interesting fact, and that is regardless of all of the measurements that had been made, and images that have been made, and the people who have done them, they were all extremely interested in way they saw things sometimes growing in water, sometimes they did not, what type of imagery would indeed reliable to detect oil, and there was an interest in a multi sensor set of experiments that they could be formed.

I found out about August of this year that there was a company named JBF Scientific that had a contract with the American Petroleum Institute to conduct a number of controlled oil spills.

dle-50

To get that permit took nearly a year and a half or so. So, I obviously tried to cooperate with these people and perform our remote sensing misssion over the set of controlled spills.

Their primary mission was the testing of dispersements, and so we did run a common combined mission--

DR. GEDNEY: What is the rate of the --

MR. CROSWELL: Well, there was a container ship that happened to go through the running. This is a container ship, these images are force, this is the saturation and recovery of it.

You do see the wake elevated, you don't see the slick. This implies perhaps that the capillary structure of the waves by which you are getting scattering , there is an X band which is a small wind generating capillary structure, and indeed is the mechanism for imagery.

The L band, gravity weight which are 15 to 20 centimeters are indeed not strongly present in this particular sea condition.

SPEAKER: What is AOL?

MR. CROSWELL: It was an oceanograph ladder (?), it was an anti-program ladder built about two or three years ago.

It has a number of transmitters, laser transmitters. It has a very broad band receiver, and a very small spectral

dlo-5 view. It can be operated in the a symetry mode and in fluorescenting mode. The fluorescenting mode was operated on this mission.

DR. GEDNEY: Gedney, Lewis. Are the Canadians paying for the SAR imagery in the processing?

MR. CROSWELL: I paid for the SAR imagery process.

DR. GEDNEY: I just wondered.

MR. CROSWELL: Yes, it is expensive. Yes sir?

DR. SALSMAN: Salsman, headquarters. Is there any postulation as to the continuation of the Rauman (?) signal? Is that absorption --

MR. CROSWELL: Absorption primarily. We are trying to get some extension coefficient measurements made. We think it is absorption.

That is, you know, we think that is what it is. I hate to make statements other than that. We really don't know, but we think that is what it is.

MR. CURFMAN: Thank you. In the opening talk that John Mugler gave he talked about the fact that we are looking for several cooperative kinds of activities and planning activities. One of these happens to be with EPA concerning non point source pollution.

During the past year the people at the Kennedy Research Center have been involved in beginning a program, a very small one on the use of Landsat data for non point

dle-52

source pollution problems, especially as they relate to non point source pollution modeling activities.

Reed Barnett from Kennedy is going to describe this activity for us at this particular time. Reed?

(Presentation of paper on Non-Point Source Monitoring by Mr. U.R. Barnett (KSC).)

QUESTIONS AND ANSWERS

DR. GEDNEY: Gedney, Lewis again. How big is the basin, a rough size?

MR. BARNETT: It is in the 20 by 30 mile range.

DR. GEDNEY: The reason I asked that, one of the biggest problems you may have using Landsat is basic resolution in plot size.

And, EPA Region five of the Great Lakes conducted a very extensive land use analysis for evaluating non-point source pollution in the great lakes, and this was for the entire Great Lakes Basin.

And, there are considerable errors introduced because of vital Landsat analysis because of the small plot sizes involved and you have to take account.

And, that error analysis will be made available probably in a month or two, and I would suggest you get ahold of that before you do extensive land use Landsat analysis.

Resolution can be a problem with Landsat in

dle-53

trying to properly classify the areas as far as land use.

MR. BARNETT: I appreciate your commenting on that problem. That is one thing, of course, we will do to assist in the analysis.

DR. KERSANDERS: Kersanders, EPA. Who at EPA are you working with on this?

MR. BARNETT: George Baily down in Athens.

DR. KERSANDERS: Oh, this is the Athens?

MR. BARNETT: Yes.

DR. KERSANDERS: Okay. You are going to be in Dallas at the end of the month, aren't you?

MR. BARNETT: That is correct.

DR. KERSANDERS: And, where is the hydrocomp located?

MR. BARNETT: Palo Alto, I believe.

DR. KERSANDERS: Palo Alto, okay.

MR. CURFMAN: Any other questions? Thank you, Reed. As we went along I think we identified, several of the speakers have identified the fact that there were several items or activities that were going on at their particular center that we were not going to discuss today.

I contacted a number of people and attempted to put together a list, and as the speakers have gone through today I find that I certainly don't have a complete list of activities that were not covered today.

dle-54

I did identify the items that you see up here as other items that are at Langley that were not really covered, and any of these activities that were talked about today.

The first one there was alluded to, the fact that there are other laboratory types of measurements that are being made in an attempt to use the laboratory measurements through various dilutions and various, more or less standard types of laboratory analysis with some rather simplified modeling to come up with ways to assess the concentration of various kinds of pollutants that have been detected such as in some of the stumping activities and things such as that.

But, there are other kinds of techniques, including additional laboratory measurements and theoretical approaches that are being used to attempt to get at some kind of a way to quantify the amount of a pollutant that is in a particular seed that we are viewing.

Also at Langley, some of you have probably heard of the activities that went on last year that was a follow on to work that was done in '75 in the Hopewell area, around the area where the Kepone tragedy occured.

We are in the process now, this fiscal year, of having essentially completed the analysis of the flow conditions and things and what the remote sensor can tell

le-55

you about some of those flow conditions, some of the dynamics in that particular river, around that particular area.

Another activity that we did not allude to in any of these presentations is the fact that we are doing some laboratory measurements at microwave frequencies.

The past year we have worked at L-Band in measuring the dielectric constant of some of the industrial waste samples at various dilutions, attempting to get at, again, what the potential might be for microwaves to discriminate between some of the various kinds of pollutants that are placed in the sea water.

Finally, in talking about the pharmaceutical waste dump waste site that was viewed, we do have some preliminary results using Langley's L-band and S-band micro wave radiometers over that site.

And, that -- those data are in the process of being analyzed and attempts are being made to correlate some of the kinds of things that were detected in that particular mission with some of the laboratory kinds of activities that are going on in microwave signature analysis. Thank you. I think that completes the water part.

CLOSING

DR. MELFI: If you notice on your schedule we had Dr. Rasool for some concluding remarks. Dr. Rasool is

not with us today. I am very glad that he was able to be with us yesterday considering his very busy schedule.

I would like to just go over some of my feelings about what we have heard in the last couple of days, and after I do that we can open it up, and if there are any additional questions or discussion, we can enter into that.

The Nimbus seven status and preliminary results, I think one of the main points that came across there was that the investigators are having difficulty in getting the data as rapidly as they expected, and we are going to be looking into that and finding out what the problems are and see if we can help the investigators get that data and get it through the system.

We fully expect to have resources in FY '80 for follow on data investigations, and you will be hearing more about that as we develop those plans.

In the stratosphere one thing that came out rather loud and clear, and that is that our people that are working in the program need to talk more with each other, and they are.

And, we will be looking at, in the next couple of months, at getting our stratospheric ozone people together so that we can get the most results with the resources that we are expending in the stratosphere. Get a better synergism if that is possible.

dle-57

In the troposphere, as we pointed out in the beginning of yesterday morning, we do have two planning activities going on; the development of the five year tropospheric plan that is being chaired by John Steinfeld (?), and we expect that we will certainly hope -- I am very optimistic that we will have a very strong trophospheric program starting in FY80.

The plan, the final plans are supposed to be out for the troposphere in the mid .May time frame. I think we had the opportunity of listening to a number of activities in the troposphere in which we are trying to better understand both the global troposphere and the regional troposphere, and I think that is moving in the right direction. That is very positive.

In the water, again we have got planning activities going on chaired by Ed Goldberg from Scripts. I know that our first workshop is next week, or the week after next.

MR. MUGLER: Next week.

DR. MELFI: Next week.

MR. MUGLER: 29 through 31.

DR. MELFI: That is right, 29 through 31 in southern California. And, I am optimistic there that we will have a very strong program in FY80.

I believe, as most of us believe, that our technology, both aircraft and space craft technology, can

and will provide some significant insight into the science of the coastal zone and Great Lakes, and in general water quality.

Maybe just a little bit of philosphy and then I will open it up for questions. As John Mugler pointed out with his cartoon slide, who are we thanking for all of the things that had happened to us last year, and at the bottom is "let me rephrase that." Just give me the hard copy John, I won't put it up.

Certainly as, I am sure, most of you are aware there has been quite a change during the last year primarily because of the desire to have a slightly different emphasis on the program.

The program in the past has emphasized, and all through the years in environmental quality, the instrument, development, and sensor development, trying to understand the physics of remote sensing techniques.

We are moving towards a slightly different emphasis in which we are working on the identification in the major scientific problems in the various areas of the environment; the water, the troposphere and the stratosphere.

And, we believe that with the identification of these scientific areas of concern that the technology that we have and the unique capability that NASA has, we

dle-59

can bring that technology and capability to bear in helping to understand the scientific problems and concerns.

And, it should get us in the best posture to work with our user agencies, EPA and NOAA, and others because they are concerned about these major problems too, or should be, and in most cases are.

So, the emphasis is, let's look at the scientific questions of concern, let's then look at our technology and what we are developing and see how that best can serve to help us answer those scientific questions, and we will have a good, sound program.

And, we started that in the troposphere with the meeting that we had last summer with John Steinfeld working with us in which we produced a document called "Major Scientific Questions in the Troposphere." John, is that about the right title, close?

And, we are going to be building on that with John chairing the project planning activity. The workshop next week with Ed Goldberg, one of the major activities that we will be doing with some NASA people and some outside scientists are identifying the major scientific issues in water quality.

And, then we will build on that with Ed Goldberg helping us in the development of the five year plan.

The next meeting of our Space and Terrestial

dle-60

Applications Advisory Committee, which is part of the Scientific Advisory Committee of the agency, is scheduled for April 30th and May 1st, and we are on the agenda to present both of these major program plans to the Advisory Committee.

We were fortunate that both John Steinfeld and Ed Goldberg are members of the Advisory Committee, and they can serve to lead the discussions about the plans that they helped us develop.

Now, without saying anything else can I ask if there are any questions or any discussion?

DR. GEDNEY: I was just wondering, has the budget mark been set for water quality in FY80, and if so how does that compare with FY79?

DR. MELFI: That is a very good question, Dick.

(Laughter.)

DR. MELFI: To the best of my knowledge the budget mark has been set for the environmental quality program which does include the water activity.

And, we are based on that budget mark, and it is just a guideline budget mark, we are expecting that the water quality activities will at least be up to the FY78 level and possibly above that, which is much better than '79.

DR. WHITLOCK: A number of people had --

dle-61

DR. MELFI: We can't hear you.

DR. WHITLOCK: A number of people have contingencies on their AN evaluations. Have the leftover money issues been settled?

DR. MELFI: All of the leftover money? Charlie we have been planning on having a meeting for the last month or so to look at the redistribution of the leftover money.

I don't really think there is going to be an awful lot of leftover money, but with the help of John Mugler, and Scott, Scott Wagner and Jack Salsman who put together our list of those things that we feel are top priority for additional resources in '79, we are prepared to support that if we do have this meeting with Greenwood. Yes?

MR. BLACKWELL: With your identification of a stronger science in the water area, does that indicate a colfax (?), of lessening of demonstration and cooperative efforts with computer agencies?

DR. MELFI: Not necessarily, Dick, because as I tried to point out if you follow it step by step we work with the scientific community and others to develop the important scientific questions.

We look at our technology, both existing and what we might be able to develop, and our unique capability,

dlc-62

and apply that those problems.

Our user agencies are also addressing important problems in the environment. So, it should match us up, you know, NASA and the user agencies so the demonstration is not being neglected.

It will be there, and it should continue to be an important part of the program. Any other questions? Yes?

DR. DRURY: I just had an observation, I think, on the mention you made in the stratosphere of the people working in the area of ozone.

I think maybe that they are communicating, it is just that they aren't in agreement yet, and that maybe what we are looking at is just a remote sensing problem that some of the other areas haven't run into yet where you are getting data from different techniques and you are trying to get a very accurate piece of information, and there are some descrepancies that perhaps some other people, you know, working in other areas haven't seen because they don't have that conflict in the data sources.

DR. MELFI: Joe, when I said communicating I didn't mean it in the sense of talking to each other because I know they do talk to each other.

I meant it more in terms of communicating to the point where we are getting some synergism, where we are

dle-63

having different groups work in a similar.

We are spending an awful lot of money in the stratospheric ozone, and if we could be getting more out of the money that we are spending, we should be getting that, and it is our responsibility to make sure we do. Yes, Dr. Tilford?

DR. TILFORD: I just wanted to add that in the case of ozone this is the first time we have really run up against the quantitative problem too, where we would like to see a half of a percent global measurement.

And, we are now looking at (maybe, at the best five percent and as you heard yesterday something worse then that in many cases.

The point of it is that we are being pushed by the regulatory agencies here to furnish them information that they think they need, and they do need, to make regulatory decisions that are multi million, or multi billion, or in a few cases multi trillion dollar decisions.

DR. MELFI: We might be a factor of 10 off in accuracy.

DR. TILFORD: So, it is a problem where we are really pushing the quantitative aspect.

Some of the other things, the 30 percent measurement is good. In ozone we would like to have better than a one percent measurement.

dlc-64

DR. MELFI: All I can see is, I can promise you that in the next few months we are going to get all of our people working in the stratospheric ozone area together and we are going to be working that problem.

And, I think it is even more important now that the upper atmospheric research program has come over and they are active, and there are things in that program that are directly relatable to what we are doing in the stratosphere. Yes?

DR. SALMAN: I appreciate your position from the headquarters standpoint, but back in the researchers standpoint, particularly water quality, I think that this type of session really doesn't do me that much good.

And the researchers in water quality, [illegible] NASA, I think would benefit more greatly from a works'op that was actually a working session rather than a show and tell.

DR. MELFI: I appreciate that comment and I think that is probably a darned good suggestion. And, it might be that in fact in the water quality area we ought to plan on having something in the next few months bringing in all of our participants from the various centers, and actually letting them roll up their sleeves in working with us.

And, I think Jack we have got representatives

from all of the centers that are going to be working with us in the development of the plan, and also with us in the workshop that Goldberg is chairing next week, and that is giving them additional opportunities to roll up their sleeves and work for the better of the program, for the betterment of the program.

So, we will keep that in mind. Any other questions or comments? Okay, thank you again for attending. It was very valuable.

(Whereupon at 5:20 P.M. January 23, 1979 the meeting was adjourned to reconvene at 8:30 A.M. the following day.)

CERTIFICATE

This is to certify that this is a true and accurate verbatim transcript of the proceedings of the Environmental Quality Program Review which took place at 10:00 a.m. on January 23, 1979, in Room 205 in the GSFC Building in Greenbelt, Maryland.

NORTH AMERICAN REPORTING

Krista J. Meyer
(Officer)

North American Reporting
GENERAL REPORTING, TECHNICAL, MEDICAL, LEGAL, GEN. TRANSCRIPTION
815 S. FREDERICK AVE., SUITE R-2
GAITHERSBURG, MARYLAND 20760
PHONE: (301) 840-1866